DISCUSSION AND CORRESPONDENCE

CONVENIENCE VERSUS FITNESS

IN recent numbers of SCIENCE a series of articles[1] has appeared pleading for the conservation of the genus as known in the early days of biology, to the sacrifice of explicitness in nomenclatural recognition of the modern increase in morphological knowledge.

As clearly recognized by Sumner[2] this is a continuation of the plea of convenience in behalf of the *status quo* that has marked recent controversies over zoological and botanical nomenclature, and is voiced by and in behalf of the same classes of objectors—the general zoologist, the amateur, the college professor, and the "true nature lover." The case of the protestants has been quite fully and ably presented in the first paper of the series, and somewhat amplified and further illustrated by the other two contributors. To reply to their points seriatim is beyond the scope of the present article, but a few words may be offered from the other point of view, that of the berated "taxonomists."

In the opening article of the series it is said:

> But there is another perennial source of confusion which has not received adequate attention. Apparently it is regarded as quite unavoidable, or perhaps it is not commonly thought of as a difficulty of *nomenclature* at all. I refer to the continual changing of names that results from the subdivision of genera. . . . And if we look for the distinctions upon which these subdivisions are based, we commonly find that the differences are very trifling indeed in comparison with the many and detailed points of resemblance between these various groups.[3]

The same author further states:

> . . . it must be borne in mind that in the Linnæan system of binomial nomenclature the generic name plays two quite distinct rôles. One of these is to designate a taxonomic group, supposed to be intermediate between the family and the species. The other is *to form the first half of the "scientific" name of each species within that group.* It is for this reason that the changing of a generic name is so much more disconcerting than is changing that of the family or order. And this is why, in the writer's opinion [he describes himself in a preceding paragraph as "one who is not a taxonomist at all"], such splitting as we have just recognized to be inevitable should be done *within the limits of the genus,* either by the creation of "subgenera," or, if necessary, by the establishment of wholly new categories between the genus and the species.[4]

In other words, any method that will avert the direful interposition of a new generic name!

The second contributor to the discussion says:

> Few zoologists ever stop to think how far we are getting away from a real binomial system of nomenclature. It is true that scientific names of animals still consist of two words, but only in a minority of cases does the first term of the binomial have any real meaning to us, or suggest ideas of a much broader and more comprehensive character than the second one. The genus name has become little more than a prefix to, or part of, the species name. . . . We learn generic names, if we learn them at all, by mere acts of memory, and we use them because we find them in the latest monographs and might be thought not up to date if we did otherwise, but what the distinctions are between these multitudes of closely allied genera we rarely stop to enquire.[5]

Notwithstanding this naïve confession, the author admits the utility of such minor divisions if they are not permitted to affect nomenclature.

> They exist in nature and should have a recognition commensurate with their importance. . . . Classification has gained in exactness and truthful representation of the facts, but through our neglect to keep the first term of our scientific names comprehensive in its application, and easily distinguished and remembered in its meaning, we have allowed our nomenclature to lose most of the prac-

[1] Sumner, F. B., "Some Reasons for Saving the Genus," SCIENCE, N. S., XLI., No. 1068, pp. 899–902, June 18, 1915. Van Name, Willard G., "Losing the Advantages of the Binomial System of Nomenclature," SCIENCE, N. S., XLII., No. 1075, pp. 187–189, August 6, 1915. Colton, Harold S., "Another Reason for Saving the Genus," SCIENCE, N. S., XLII., No. 1079, pp. 307, 308, September 3, 1915.

[2] *L. c.*, p. 899.

[3] Sumner, *l. c.*, pp. 899, 900.

[4] *Ibid.*, p. 900.

[5] Van Name, *l. c.*, p. 187.

tical advantages and conveniences of the Linnæan system.[6]

The author of the third article cited above places great stress upon the fact that "It is by genera that animals and plants are catalogued," and considers that "this whole discussion hangs on the question, is it necessary to change generic names to advance our knowledge?" He goes on to reiterate:

> In conclusion, generic names are those by which animals are catalogued, therefore should not be changed without overwhelming evidence in favor of the change. This value of the generic name has not been sufficiently emphasized.[7]

These three writers (for they all harp essentially on the same string) seem, despite all their admissions, really to forget that increase in knowledge leads in all fields of scientific progress to the introduction of new technics. It is not only necessary to learn new facts but new terms for their expression. In the good old days of the last half of the eighteenth century and the early part of the nineteenth, zoological genera were few; and when those founded by the great Linnæus proved, in the opinion of his immediate successors, to be inadequate to satisfactorily meet the requirements of their new discoveries, they proposed what were in fact new generic groups, but in deference to the past apologized for their seeming disrespect of the *status quo* and demurely called them subgenera, to break the shock of their seeming irreverence. Yet as years passed these groups gradually took their place in the systems as valid genera, and more were constantly added. The old Linnæan genus *Mus* included at first marmots and flying squirrels, as well as all the rat- and mouse-like animals then known. All known deer and antelope were each included in a single genus, and so on through many other groups.

More than one eighteenth century genus has since been distributed into several *families*, to say nothing of genera. And what an inconvenience this must have been for the "general zoologist" to have to learn so many new generic names! What a trouble it must have been too for the cataloguer! But such is the history of science, and who is to say when we have genera enough, and how many shall be weeded out as merely useless and confusing, and how many more may be conserved as subgenera, and thus save the present-day overworked "general zoologist" and his fellow sufferers from knowing that such divisions and names have ever been proposed by the poor specialists who were so misled in their researches as to think them necessary.

I must confess, however, that I share the weaknesses of my class, the specialists, in believing that the primary function of nomenclature is to express the facts of classification, not to conceal them. The old genus *Sciurus*, in its early sense, comprised all squirrel-like animals of all parts of the world. In its old sense it would now comprise several hundred species, all looking near enough alike to be called squirrels, yet containing a score or more natural groups, sharply defined geographically and by minor but not unimportant morphological characters. Many of these minor groups are now currently given the rank of genera, others stand as merely sections or subgenera. Arranged thus in a monograph or in a systematic catalogue their various degrees of relationship and their interrelationships might be approximately expressed, but incidental references to them under the single generic name *Sciurus* places all on the same level, with no clue as to whether they are closely or remotely related, or to the kind of squirrel intended. If on the other hand they are mentioned under their modern group names the specialist knows, and the general zoologist should know, exactly their relationship to other squirrels, in other words, what kind of a squirrel is indicated. But this is apparently of no importance to advocates of "convenience" as the prime factor in every matter relating to nomenclature.

An intelligible compromise would be the use of both the generic name (in the broad sense) and the subgeneric name (in parenthesis) in incidental references. But this would be intolerable in the general zoologist, as it would, in the case of subspecies, involve in effect a quadrinomial nomenclature, and a further departure from the primitive binomial of the

[6] *Ibid.*, p. 187.

[7] Colton, *l. c.*, p. 308.

good old times of thirty to fifty years ago when (to quote Van Name), "a genus name had in those days a real meaning to some others besides the specialists in the class of animals to which the genus happened to belong."[8]

It is of course to be admitted that there are good genera and bad genera; that many groups have been proposed as subgenera or even full genera on inadequate grounds. Our synonymies show what has been the fate of many of them, and a like fate doubtless awaits many, of recent origin, that have still to be weighed in the balance of concurrent approval. As the value of characters is a question that can not, from its nature, be made the subject of rules, as can questions of nomenclature, there seems only the slow relief afforded by time and the concurrent judgment of the specialists of each field for the evils of too much subdivision.

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THE INHERITANCE OF CANCER

IN a short note[1] I have recently commented on Dr. Maud Slye's work on the inheritance of cancer in mice. As to the credit due Dr. Slye for her careful and laborious experiments there can be no question. The importance of the subject, however, is such that it is essential to understand the exact distinction between the gathering of valuable data and the interpretation of such data when gathered.

The impression that Dr. Slye believed that cancer was inherited in a Mendelian fashion appears to have been more or less generally created by her paper already mentioned. Any one reading the editorial on her work in the *Journal of the American Medical Association* (Vol. 64, p. 1,326) can not fail to see that the "great laws" of heredity mentioned there are intended to be the Mendelian laws. The whole subject is treated from that standpoint and the optimism apparent must be considered to be chiefly due to the belief that Dr. Slye's work is an example of Mendelian inheritance.

So too, any one reading the review of recent work on cancer research by Dr. W. A. Dennis in the *St. Paul Medical Journal* (Vol. 17, pp. 494–500) can see that he believes that

> . . . the importance of her findings lies in the fact that hereditary transmission . . . is not fortuitous but that, given parents of pure breed the results of crossing may be confidently predicted.

After outlining correctly the basic principles of Mendelian inheritance, with the cross of albino and gray mice as an example, Dennis goes on to say:

> Maud Slye has taken advantage of this law [Mendel's] of heredity to study the transmissibility or inheritability of cancer in mice. . . . These studies [Slye's] have shown that the appearance and numerical value of the albino character can be predicted with certainty from the manner of mating the parents. The same is true of the whirling character of the Japanese waltzing mouse *and the same has been demonstrated to be true of cancer.*[2]

The fact that no correction of the impression so created was apparently forthcoming, and the fact that the diagrams in Slye's paper showing the inheritance of albinism represented a hitherto undescribed type of heredity led me to comment on her work.

Slye's recent denial of any desire or intention to apply a Mendelian interpretation to her experimental results is an extremely important postscript to her paper since it makes it virtually impossible to expect the exact numerical predictions in crosses which her reviewers have believed could be made.

Further than this, Slye's beliefs as to the inheritance of albinism are, as I have stated before, at sharp variance with the experimental results of Castle, Allen, Bateson, Durham, Cuénot, Plate, Davenport and others. The suggestion made by Slye[3] that the utilization of wild grays rather than "artificial laboratory" grays places her work in a position different from that of these other investigators is not significant, for I have repeatedly used wild grays in my crosses and have found that their hybrids obey Mendel's law in respect to the color characters which they inherit.

I have suggested that Dr. Slye's data show-

[8] *L. c.*, p. 187.

[1] SCIENCE, N. S., Vol. 42, pp. 218–219.

[2] *Italics mine.*

[3] SCIENCE, N. S., Vol. 42, pp. 246–248.